volume that the matter treated therein is a "continuation of the subjects already discussed."

It is true that many perfume-yielding plants treated of in the first volume are again referred to in the second. Mr. Sawer says that the matter published by him two years ago is now as far as possible brought up to date. This may be so, so far as the chemistry of the various substances is concerned; but we scarcely think, as an instance, that the paragraph on Ambrette (p. 402) indicates any new discovery since 1892, when *Hibiscus Abelmoschus* was briefly referred to and sufficiently described. The following is the paragraph to which we refer. It is placed in Section iv. under the head of "Addenda to Volume i.":—

> "Ambrette, the seeds of *Hibiscus Abelmoschus* (Lin. Spec. 980).—'Hibiscus' is one of the names given by the Greeks to 'Mallow,' and is said to be derived from *Ibis*, a Stork, a bird which is said to chew some of the species. 'Abelmoschus' is derived from the Arabic *Kabb-el-Misk*, 'grain or seed of musk.' The 'Mallow' group consists of a very large genus of *Malvaceæ*, characterised by their large showy flowers being borne singly upon stalks towards the ends of the branches, by having an outer calyx or involucel composed of numerous leaves, and an inner or true calyx cut into five divisions at the top, which does not fall away after flowering; by having five petals broad at top and narrow towards the base, where they unite with the tube of the stamens; and by the latter forming a sheath round the five-branched style and emitting filaments bearing kidney-shaped anthers throughout the greater part of its length. The fruit is five-celled, with numerous seeds. *Hibiscus Abelmoschus* is a shrub of 6 to 8 feet in height, native of the East Indies and South America. Its leaves are somewhat peltate, cordate, 5 to 7-angled, acuminated, serrated, stem hispid; pedicels usually longer than the petioles; involucel 8 to 9-leaved. Flowers sulphur-coloured with a dark blue centre. Capsules conicle (*sic*) covered with bristles. The seeds are large and have a very musky odour. The seeds yield on distillation 0·1 to 0·25 per cent. of essential oil, which congeals at + 10° C. Its sp. gr. at 25° C. is 0·900 to 0·905."

It will scarcely be conceded that the bulk of this paragraph brings the information on Ambrette up to any more recent a date than might have been given in the book issued two years ago, for the botanical description is acknowledged as having been obtained from such works as Rumphius' "Herbarium Amboinense," Rheede's "Hortus Malabaricus," &c. The descriptions, indeed, of this and of most other plants referred to is of little or no value in a book of this character, and only helps to increase its bulk, a thing to be avoided in a work "intended for the use of growers, manufacturers, and consumers."

The range of plants over which Mr. Sawer travels is very extensive, and is indicated by the five and half pages of works consulted, as well as by a casual glance through the pages of the book. Many of these plants are new to us as perfume yielding.

The three species of *Barosma*, for instance, namely *B. crenulata*, *B. betulina* (which, by the way, is spelt Barasma), and *B. serratifolia*, the leaves of which are well known in pharmacy under the name of Buchu leaves, seem as much out of place in a work on perfumes as asafœtida or castor oil would be; and the fact that the leaves of the Barosmas "are used by the Hottentots as perfumes," is but a slight recommendation for their adoption in civilised life. Notwithstanding that Mr. Sawer summarises rather fully what has been done by such well-known chemists as Prof. Flückiger, Messrs. Schimmel, and others, in the examination of Buchu oil, he does not inform us whether the oil has been actually used in perfumery, or whether there is any prospect of its becoming an article of the perfumer's trade. All we gather on this point is that its odour agrees with that of peppermint.

Regarding the arrangement of the plants or products referred to in the book, we cannot discover that any systematic method has been attempted; the plants are not classed scientifically nor alphabetically, and if it were not for a fairly extensive index a good deal of difficulty would be experienced in finding any particular plant required. It cannot be denied that the book contains an immense amount of useful and interesting matter, and exhibits an enormous labour expended in its compilation. With a good deal of judicious pruning, a systematic classification of subjects, and much careful editing, the two books might be reduced into one good-sized volume, and made a standard work on all matters relating to perfumery.

That a careful revision has not been made of the proof sheets, is evident from the frequent mis-spelling both of scientific and common words. Thus we find *Melliaceæ* for *Meliaceæ*, String Bark for Stringy Bark, *Stellingia sabifera* for *Stillingia sebifera*, Madagasca for Madagascar, and such like errors that might with ordinary care have been avoided.

OUR BOOK SHELF.

Introduction to Elementary Practical Biology. By C. W. Dodge, Professor of Biology in the University of Rochester, U.S.A. (New York: Harper and Brothers, 1894.)

THIS volume of 422 pp. octavo is the first laboratory book on the established lines of Huxley and Martin's "Elementary Biology" which has reached us from the New World. It, however, excels that in scope, owing to the introduction of additional types of both plants and animals—the Starfish, Locust, Sponges, Rock-weed (*Fucus*), Liverwort, and Water-silk (*Spirogyra*) being among those dealt with. The work embodies the results of seven years' experience in practical teaching; but, that notwithstanding, it bears at every turn the impress of the recognised English treatises of its kind, and to these the author, unlike certain writers nearer home, manfully acknowledges his indebtedness. The book opens with an introduction, dealing with instruction in manipulation and the use of instruments, and closes with an appendix, giving lists of and recipes for reagents, and there are added a bibliography of works of reference and a very good glossary-index. The bulk of the volume is subdivided into three parts dealing in succession with the Biology (1) of the Cell, (2) of the Animal, and (3) of the Plant, elementary experimental physiology and the study of habit receiving adequate attention. Such novelty as is claimed for the work is born of its author's conviction that "the methods of teaching now in vogue for elementary classes are methods of *instruction* rather than of *education*" (!) and he sets himself to overcome this imaginary defect of what he terms the "verification method" by the introduction of questions, as opposed to the more diadactic statement of facts customarily resorted to. Up to a certain point this may be all very well. For example, in dealing with

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the Protozoa, with which and the analysis of simple cell structure the author's course commences, the student, being told how to capture and mount his sample, is asked, "How many different shapes can you distinguish?" "What variations in size?" "In color?" and other questions of like order; but when there follow these (on p. 5 of the work), "How do these animals eat?" "Digest their food?" "Breathe?" (*sic*) we confess to a feeling of sympathy with the befuddled beginner. And when, further, after an altogether insufficient preamble and at the outset of his inquiry into the wide domain of biology, the tyro is asked, of the Amœba, "Is the process [of fission] preceded by sexual union?" "How is one sex distinguished from the other?" and, *à propos* of the cerebral hemispheres of the frog, "Why are they called hemispheres?" one's sympathy gives place to pity for the student thus led astray. We entirely disagree with the author's dictum that sooner or later the student will have to learn to use the microscope, and it matters little when he does so; and we further doubt the advisability of his interrogatory method, when "the questions usually apply equally well to several related forms," particular species being said to be "not required." A training in elementary biology is one in manipulation in a field beset with snares and pitfalls, rendering it a primary necessity to teach the beginner what to leave unconsidered. However, the experiment, while not altogether new, is an interesting one; the book is carefully compiled, and we await with interest the verdict of time upon the system which it advocates.

Notes on the Ventilation and Warming of Houses, Churches, Schools, and other Buildings. By the late Ernest H. Jacob, M.A., M.D. (London: S.P.C.K., 1894.)

A MELANCHOLY interest is attached to this little manual of health in the fact that its gifted author passed away on March 1. His posthumous work shows what a promising life was cut short, and will serve as a memorial to him. The idea that human beings confined in public buildings should have pure air at a suitable temperature supplied them, has only in recent years been taken seriously. It is notorious that in most churches there is no attempt at proper ventilation, and they are only excelled, as far as disregard for the laws of health go, by many Nonconformist chapels with galleries, and mission-rooms created by knocking two cottages into one. Dr. Jacob's manual should be consulted by those who are responsible for such buildings. Therein they will find described the general principles by which buildings are rendered healthy. The book should also be read by the householder, for he will learn from it how an ordinary dwelling-house ought to be ventilated and better, will find that it is an easy and not very costly business to make the average English house less stuffy and more healthy than it usually is. Indeed, all who desire in a popular form information on the subject of ventilation, should procure this book, while architects and builders would benefit the community by taking its lessons to mind.

LETTERS TO THE EDITOR.

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Rotating Shafts.

IN your account (NATURE, May 10, p. 43) of Dr. J. Hopkinson's "James Forrest" lecture at the Institution of Civil Engineers, appears the following statement: "Another example, having a certain degree of similarity with the case of struts, is that of a shaft running at a high number of revolutions per minute, and with a *substantial* distance between its bearings. . . . How will the shaft behave itself in regard to centrifugal force as the speed increases? *In this case, so long as the shaft remains absolutely straight it will not tend to be in any way affected by the centrifugal force,* but suppose the shaft becomes slightly bent, it is obvious to anyone that if the speed be enormously high this bending will increase, and go on increasing until the shaft breaks. In this case also we may use mathematical treatment; we find that the condition of the shaft is expressed by a differential equation of the fourth order, and from consideration of the solution of this equation we can say that if the speed of any particular shaft be less than a certain critical speed, the shaft will tend to straighten itself if it be momentarily bent, but that, on the other hand, if the speed exceeds this critical value, the bending will tend to increase with the probable destruction of the shaft." (The italics are mine.)

The italicised statement seems to imply that a certain operating cause may have absolutely no effect, which cannot of course be the meaning Dr. Hopkinson intended to convey. Most engineers, it is to be hoped, are aware that the natural tendency of the material of the shaft is to retire from the axis of rotation, and that this is *necessarily* associated with a state of strain and stress throughout the shaft, whether straight or bent, for all speeds of rotation. Dr. Hopkinson must, I think, have had his mind so fully occupied with the idea of rupture through instability, worked out by Prof. Greenhill,[1] that he overlooked the fact that his language suggests the non-existence of the more commonplace and essential elastic phenomena.

So far even as rupture is concerned, Dr. Hopkinson's statements are, I believe, incomplete. The ordinary strain and stress developed by rotation in a shaft may, as I have shown elsewhere,[2] exceed the limits of safety before a velocity is attained at which, on the Greenhill theory, instability becomes possible. This is the more likely to happen the shorter the cylinder and the thinner its walls, if it be hollow; but even in a solid iron cylinder of length eight or nine times its diameter—a very substantial distance in a thick cylinder—the strain developed would be such as to merit an engineer's careful attention before a critical velocity was reached associated with instability.

I am somewhat doubtful whether Dr. Hopkinson's remarks on instability itself are altogether satisfactory. On the mathematical theory there appear in reality to be a series[3] of critical values, if any, at which instability *may* occur. Supposing the velocity gradually raised, it seems possible, *theoretically*, for the shaft to safely surmount the first crisis. It then would appear to remain unexposed to instability until the approach of the next higher critical velocity, and so on.

As Dr. Hopkinson says, Prof. Greenhill's instability theory leads to a differential equation of the fourth order. The solution of this equation is, however, dominated by the terminal conditions[4], at the ends or bearings of the shaft, and unless these be correctly assigned the numerical results deduced from the theory are untrustworthy. This is, I think, one of those points where the practical experience of the engineer is a most essential auxiliary to the analysis of the mathematician.

Kew Observatory, May 11. CHARLES CHREE.

IN order to shut out every possibility of ambiguity, I might have said, instead of "substantial distance between the bearings," "distance between the bearings very great in comparison with the diameter of the shaft," and in the next sentence quoted it would perhaps have been clearer if I had said "will not be broken by centrifugal force." But I do not think that in fact I could be misunderstood by anyone.

It was hardly desirable that I should touch upon the terminal conditions, or upon the possibility of stability between the critical values, in a paragraph introduced for illustration and not for detailed information.

But Mr. Chree's letter does remind me that I neglected to refer to Prof. Greenhill's name in this connection. This I should have done with the greatest pleasure, but, unfortunately, for the moment I forgot that it was he who had worked on the problem. I write this note in order to make the acknowledgment. J. HOPKINSON.

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[1] Institution of Mechanical Engineers, *Proceedings*, 1883, pp. 182–209.

[2] Camb. Phil. Soc. *Proceedings*, Feb. 8, 1892, pp. 283 *et seq.*

[3] See *Phil. Mag.* August 1892, pp. 166-67.

[4] See Camb. Phil. Soc. *Proceedings*, *l.c.* p. 300, and *Phil. Mag. l.c.* pp. 164-65.

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